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Educational differentials in adult mortality in low- and middle-income countries

Bruno Masquelier[1], Alessandra Garbero[2]

Abstract

In high-income countries, there is extensive evidence showing that higher levels of educational attainment, higher income and higher occupational classes are correlated with lower risks of dying among adults. Far less is known on mortality differentials in low- and middle-income countries due to the lack of information on the socio-economic characteristics of the deceased.

In this paper, we evaluate whether survey reports on the survival of sisters can help to fill this gap. We find that levels of educational attainment are correlated within families, and therefore characteristics of the deceased can be inferred from those attained by their sisters responding to the survey. Because estimates of adult mortality based on sibling histories have large confidence intervals, we pool all DHS together and fit a generalized liner mixed effects models to capture mortality differentials. In most surveys, higher education is associated with lower risks of dying, especially in urban areas, but reversed gradients are observed in several countries with high mortality and generalized HIV prevalence. There is limited support for the hypothesis that this puzzling pattern results from larger underreporting of deaths by women with less education. Instead, we argue that sibling histories truly reflect the complexity of the relationship between adult mortality and education, especially in countries affected by the HIV/AIDS epidemic. Overall, the sibling approach offers new possibilities for measuring inequalities in adult mortality in low- and middle-income countries.

Keywords: Inequalities in mortality; Adult mortality; Siblings; Educational attainment; Demographic and Health Surveys

In high-income countries, the literature has largely documented the existence of an education gradient in adult mortality and morbidity (Kitagawa and Hauser 1973; Kunst and Mackenbach 1994; Preston and Elo 1995; Zajacova 2006; K.C. and Lentzner 2010; Hummer and Lariscy 2011). The gradient has widened in recent decades in the Russian Federation, in Western Europe and in the United States, and

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this adverse trend has rekindled interest in inequalities in mortality (Mackenbach et al. 2003; Murphy et al. 2006; Meara et al. 2008). By contrast, far less is known on inequalities in adult mortality in low- and middle-income countries, due to education, income or occupation. Maternal education has been established as the single most important determinant of differences in child survival (Caldwell 1979; Fuchs et al. 2010), but there is limited evidence on the persistence of these differentials in adulthood. Some studies conducted in Asia document the usual negative relationship between education and adult mortality, with a few exceptions, primarily related to deaths due to breast cancer or ischemic heart disease (K.C. and Lentzner 2010). In Sub-Saharan Africa, comparable studies on mortality differentials remain rare (Berhane et al. 2002) and results on morbidity gradients are more mixed, especially as far as the link between HIV infection and education is concerned (Fortson 2008).

This lack of conclusive research is mainly due to the incompleteness of vital registration systems, which makes it difficult even to estimate the overall level of mortality in many low- and middle-income countries (Mathers et al. 2005). Measuring differentials is further complicated by the quasi absence of questions on the socio-economic characteristics of the deceased in censuses and surveys. In addition, few demographic methods developed for incomplete data lend themselves to the measurement of differentials. For example, methods based on stable population theory and designed to estimate the completeness of death reporting in censuses or vital registration cannot easily be applied to sub-populations, especially when these sub-populations experience varying rates of migration (Bennett and Horiuchi 1984; Hill 1987). Intercensal survival methods - the second avenue to estimating adult mortality - are also ill-suited to study differentials (Preston and Bennett 1983). Typically, with intercensal survival methods, the number of women aged 50-54 in a census conducted in 2010 would be compared with the number of women aged 40-44 in a census conducted ten years earlier for each level of educational attainment. When reconstructing populations by age, sex and level of educational attainment since 1970, Lutz et al. (2007) used this approach; they followed up women aged 40-49 across several censuses in 7 developing countries and France. They found an average increase in the life expectancy at age 15 of one year from women with no education to women with primary education, and of two years from women with primary education to secondary education. This was later used to compute education-specific mortality rates to develop projection of populations by level of educational attainment (KC et al., 2010). However, intercensal survival techniques are highly sensitive to age misreporting and changes over time in the completeness of the enumeration (Timæus 1991). They also require that the population is closed to migration or that age-specific rates of net migration are available for the intercensal period. All these problems introduce errors which may greatly vary with education.

A third range of methods is probably more adequate to study differentials; those based on data collected on the survival of close relatives, that is, parents, spouses, or siblings. For socio-economic characteristics that are highly correlated within families, the characteristics of those who provide information can serve as proxies for the characteristics of their relatives. Timæus (1984) adopted this strategy with data on the survival of parents collected in a fertility survey conducted in Lesotho (1977). He found consistent differentials by level of educational attainment; the life expectancies at age 15 of parents of respondents with no formal education was about 6 years lower than among parents of respondents with secondary or higher education. However, differentials derived from orphanhood data are difficult to interpret because the parent-child correlations in educational attainment are affected by time trends such as a rise over time in education levels: if the distribution of educational attainment is considerably modified, the characteristics of the children can be loosely related to the education of parents. Because of the smaller age difference between spouses, and the potential indirect effects of characteristics of spouses on mortality (Jaffe et al. 2006), data referring to the survival of first spouses could be used as well. Unfortunately, attempts to estimate mortality rates from widowhood data have been discouraging so far, especially when using men's reports (Timæus 1987; Makinson 1993; Bobak et al. 2002). Widowers seem to underreport previous marriages or conceal the death of their partner (Malaker 1986). Conversely, respondents may prefer to report a deceased partner rather than a divorce when divorces are not socially accepted. In sum, data on parents or spouses are not very promising to explore differentials. There is a third type of relatives for which survivorship statistics can be computed: brothers and sisters. More than 110 Demographic and Health Surveys (DHS) have included questions on siblings of women aged 15 to 49 since the early 1990s. These sibling histories are now central to the measurement of adult and pregnancy-related mortality in low- and middle-income countries (Rajaratnam et al. 2010; Reniers et al. 2011).

In an inspiring paper entitled "The familial technique for linking maternal death with poverty", Graham et al. (2004) suggested using sibling histories to go beyond levels and trends in mortality and look into inequalities in mortality. They contended that the educational level or the poverty status of women interviewed in DHS can serve as proxies of their sisters' characteristics. Quite surprisingly, this familial approach has since been seldom used, expect by Damien de Walque and colleagues. With DHS data from Cambodia and several countries in Africa, de Walque showed that the educated adults were more likely to be killed in conflicts than other segments of the population (de Walque 2005; De

Walque and Filmer 2012; de Walque and Verwimp 2010). De Walque and Filmer (2013) also performed a systematic analysis of DHS sibling histories and presented mortality gradients by level of educational attainment and urban/rural residence. They found the expected inverse relation between adult mortality and educational attainment. They showed that education-based gradients had widened over time in countries affected by HIV/AIDS, especially for males. Their results, however, were obtained by pooling together DHS surveys in models separating countries into three broad groups: (1) Sub-Saharan African countries with low HIV prevalence, (2) Sub-Saharan African countries with high HIV prevalence and (3) countries in other regions. Their analysis thus obfuscates the heterogeneity in mortality gradients across countries. De Walque and Filmer (2013) concluded that "on the whole, the data do not show large gaps by urban/rural residence or by school attainment" (p.23). This finding is somewhat at odds with other studies conducted in low- and middle-income countries in which mortality differentials by education appear to be as pronounced as in affluent societies (Berhane et al. 2002; Huong et al. 2006; Hurt et al. 2004).

The present article focuses more on the methodological aspects of the "familial approach" than on substantive conclusions. Its purpose is to establish whether sibling data can provide valuable insights on the direction and magnitude of inequalities in adult mortality related with education. We address four methodological questions:

(1) Is a woman's educational level a good proxy for those of her sisters in low- and middle-income countries?
(2) Can sisters' reports shed light on cross-country variations in the mortality gradients by educational levels?
(3) Is there some evidence that the quality of death reporting varies across educational categories, introducing biases in mortality differentials?
(4) Can DHS data support an analysis of trends in mortality gradients?

We focus on educational attainment for two reasons. First, it is a stable characteristic across the life span, and only a very small fraction of the population has not reached their highest educational level by the age of 25 in low- and middle-income countries. Therefore, when working on mortality above age 25, we avoid the problems associated with status changes, as they may arise in an analysis based on occupations or households' economic status. Second, educational attainment is highly correlated within groups of siblings. In high-income countries, approximately 40 to 60% of the overall variation in years of schooling can be accounted for by factors shared by siblings (Björklund and Salvanes

2010). Dahan and Gaviria (2001) also found high correlations of schooling outcomes among siblings in Latin America.

1. Sibling histories collected in DHS

Our analysis is based on 111 standard DHS conducted in 47 low- and middle-income countries[3]. This study essentially covers countries in sub-Saharan Africa (82 DHS), but also includes other countries such as Brazil, Haiti, Nepal, Cambodia, Timor-Leste, or Bolivia.

DHS are nationally-representative surveys comprising three main questionnaires; (1) a list of all household members collecting basic information and household characteristics, (2) a detailed individual questionnaire designed for women of reproductive ages, and (3) a men's questionnaire administered in a subsample of households. From the early 1990s, a "maternal mortality" (or "adult mortality") module has been included in some women's questionnaire and a few men's questionnaire. A standardized set of questions is used to elicit an exhaustive list of siblings born to the same mother. Information is collected by birth order about their gender and survival status. Current age is recorded for surviving siblings, while age at death and years since death are asked for the deceased. Direct mortality estimates (occurrence / exposure rates) can thus be obtained from sibling reports. Additional questions identify pregnancy-related deaths. At this exploratory stage of our research, the analysis is restricted to female mortality. This is because sibling histories are mainly collected from women aged 15-49; only a dozen of surveys have included questions on siblings in the men's questionnaire. Women's reports on their brothers could be used but inferring levels of educational attainment from the respondents is more problematic for siblings of opposite sex.

In the absence of a gold standard, it is difficult to evaluate how well sibling histories do in estimating adult mortality. A handful of studies compared them with estimates from the United Nations (Gakidou et al. 2004; Stanton et al. 2000; Masquelier et al. 2014). They all conclude that DHS data tend to underestimate mortality, because deceased siblings are disproportionately omitted and some deceased siblings are reported as alive. In a validation study of DHS in Senegal, nearly 25% of female respondents did not report deaths of one of their adult sisters (Helleringer et al. 2014). Other data quality assessments are provided by Stanton et al. (2000) and Masquelier (2014). Moultrie et al. (2013)

[3] We discarded several DHS: (1) the DHS conducted in Nigeria in 1999 because the data are known to be of poor quality (Pullum 2008), (2) DHS São Tomé and Príncipe in 2008/9 because the sample size is too small to produce reliable estimates, and (3) DHS Mozambique 1997 because the percentage of deceased siblings with missing or unknown age at death was as high as 24% and the percentage of deceased siblings with unknown or missing information on the timing of deaths was as high as 59%. In addition, 4 DHS with sibling data that are not distributed in the public domain (Eritrea 1995, Guinea 1992, Mauritania 2000/1, South Africa 2003).

also discuss some limitations of sibling histories. Despite these limitations, for reasons stated above, sibling histories are one of the only available options to explore mortality differentials in adult survival in countries without comprehensive registration of death.

2. Is a woman's educational level a good proxy for those of her sisters?

Our first question is whether the level of education of a respondent may be used as a proxy for that of her sisters. Ideally, we should match adult sisters together (across households) and compare their levels of educational attainment. This is impossible, because many sisters do not live in the sampled household and even those who do are not easily identified in the DHS questionnaire. Kinship ties can only be established through the relationship with the household head, or, for children aged less than 15 (sometimes 18), through the identification number of their mother and father if they live in the same household. Therefore, there is very limited information in DHS to link together adult sisters who have completed their education.

We address this issue by focusing on women reported as daughters of the household head. For each household counting at least two of these (above age 18), we randomly select a pair of sisters and compare their level of education. In accordance with the classification used in DHS, educational attainment is recoded in three categories: (a) no education, (b) primary education, (c) lower secondary education or higher. We first compute the proportion of women who are classified in the correct category if the level attained by her sister is used as a proxy for her own. On average, 70% of women are correctly classified when using data from one of her sisters. This proportion will obviously be higher in settings with small inequalities in educational attainment, so we also compute a weighted Kappa coefficient to take into account the agreement occurring solely by chance. 10% percent of surveys have only a fair agreement on educational attainment of two randomly selected sisters (<= 0.4). 46% of surveys have a moderate agreement (0.41 – 0.6), 42% of surveys have a substantial agreement (0.61 – 0.8), and 2 surveys have an almost perfect agreement. The scope of mismatching is probably larger, because we retained daughters of household heads who cohabit with each other, which is a more homogeneous sample than all sisters. Yet, in the absence of direct measurements, it seems wise to use the respondent' educational attainment as proxy for that of her sisters. Random misclassification will dilute the mortality gradients, because the different groups will be more similar to each other than in the absence of misclassification. Wide age ranges between sisters will also compromise the quality of the approximation because older respondents are likely to be less educated than their younger sisters.

The reverse is true for younger respondents compared with their older sisters. Ideally, the distribution of age differences should be symmetrical, centered around zero, and as narrow as possible. For this reason, we discard deaths and exposure time below age 25 and above age 40 for the remaining of the analysis[4].

3. Can sisters' reports shed light on cross-country variations in the mortality gradients ?

We now turn to mortality gradients associated with education. To compute mortality rate ratios, the original data sets which have one row for each woman aged 15-49 need to be reshaped so that the total number of deaths and person-years of exposure time among sisters can be computed for the period 0-6 years prior to the survey (data for periods more distant in the past are discarded for reasons explained below). This procedure is described in details in Moultrie et al. (2013). Once the person-years and deaths have been aggregated across surveys and weighted according to the sample weights, a regression analysis can impose some constraints on mortality rates. Various strategies have been employed in previous work, including logistic regression (Obermeyer et al. 2010), Poisson regression (Timæus and Jasseh 2004), and generalized additive models (Masquelier et al. 2014). However, all these approaches were designed to capture mortality trends at the national level, and our objective here is to quantify the variation among surveys in the effect of education on adult mortality.

We use a Poisson mixed-effects model, which incorporates both fixed-effects parameters and random effects associated with each survey in the linear predictor. An unpooled regression would produce noisy estimates with large confidence intervals, but this model will "shrink" the estimates toward the overall mean, especially when the surveys have a small sample size. Another way to put it is to say that surveys will "borrow strength" from each other. To simplify the analysis, we only compare women whose sisters had no education or had only reach primary school with women whose sisters had been to secondary school or higher. The coefficient associated with the educational level among the fixed effects, once exponentiated, corresponds to what can be called the *overall mortality rate ratio* (the overall effect of education). Among the fixed effects, the regression model also includes the log of exposure time as an offset parameter and the age group of sisters. The level and age pattern of mortality are assumed to remain constant over the 0-6 years period. The random effects consist in a survey intercept and a varying slope for the educational level of respondents. This varying slope is supposed to capture variations across surveys in mortality gradients. Because residents from urban

[4] No restriction is imposed on the respondents, however. This is because women aged 25-29 should also be reported by younger sisters aged 15-24 - otherwise a negative skew in the age differences would be introduced. Conversely, women aged 35-39 should also be reported by older sisters.

areas have a higher educational attainment and the effect of education on adult survival could differ across type of residence, we also stratify the regression by urban/rural status of the respondents. The model is fit with the *lmer* package of R statistical software (www.r-project.org).

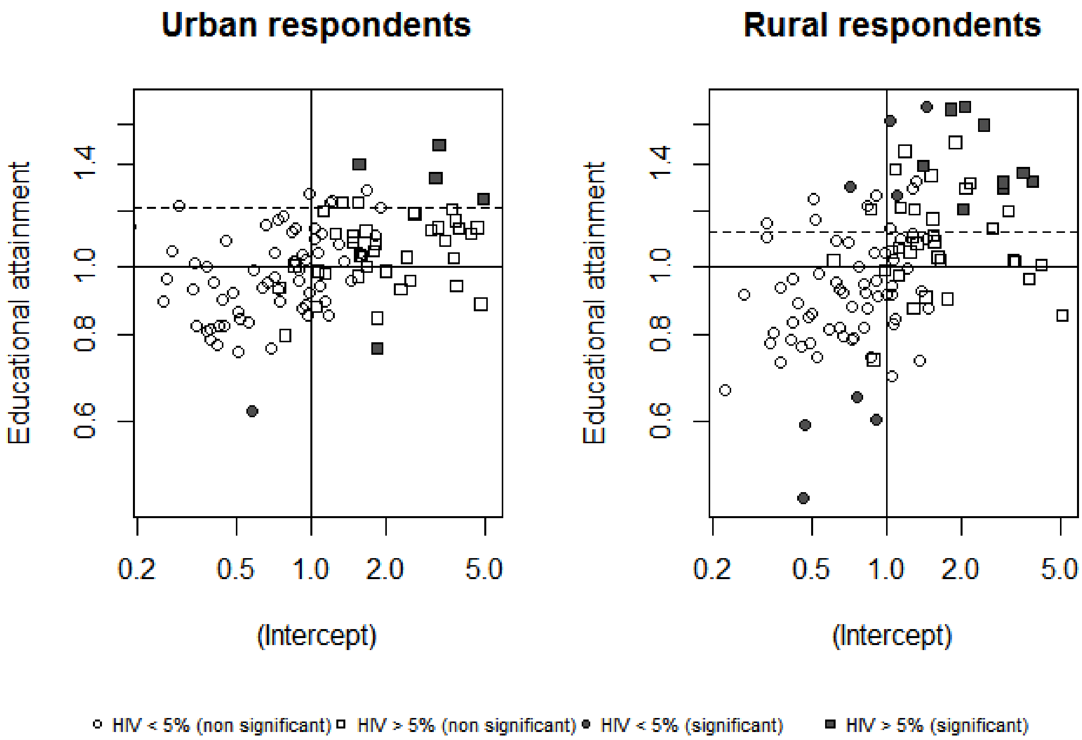

Figure 1: Plot of the conditional modes of the random effects (exponentiated) showing the deviations to the overall mortality rate ratio associated with education.

This model reveals several general patterns. First, in both rural and urban areas, the *overall mortality rate ratio* for educational level is lower than 1 (estimated respectively at 0.89 and 0.82) and both coefficients are significantly different from unity at the 0.05 level. As expected, this means that women with higher educational level face lower risks of dying overall. Second, the mortality advantage for women with higher education is larger in urban areas, presumably because of greater disparities in access to health care. In other words, more educated women could be better able to take advantage of health services available in urban areas. Third, there is substantial heterogeneity in mortality rate ratios. This is illustrated in Figure 1, which plots the conditional modes of the random effects for the fitted model (these modes are exponentiated to facilitate the interpretation). Values on the x-axis reflect the extent to which each survey departs from the overall mortality *level*. Surveys located to the right (above 1) are those where the mortality level is higher than the expected from the fixed effects. The estimated values are represented with squares if the survey was conducted in a country where HIV prevalence had reached at least 5%, and with dots if the HIV prevalence was lower at the time of the survey (UNAIDS 2012). The y-axis indicates how the mortality rate *ratios* vary across surveys. Values that are higher than 1 correspond to mortality rates ratios that are higher than the overall effect, that is, they indicate that the mortality advantage of education is smaller in this particular survey. Some values are located above the dashed lines; these correspond to cases where, when combining the fixed and random effects, the mortality rate ratio turns to the advantage of less educated women (hence a reversed gradient). In addition, surveys where the mortality gradients differ *significantly* from the overall mortality rate ratios (as computed from prediction intervals on the random effects) are indicated with dark dots and dark squares. In urban areas, this is the case for only 6 surveys, 4 of which suggesting that more educated women are disadvantaged. Based on reports from rural residents, 17 surveys differ significantly from the overall mean, 13 of which are above the dashed line indicating higher risks of dying among more educated women. Interestingly, these surveys are almost all located in the upper half of the distribution of probabilities ${}_{15}q_{25}$, and a majority of them were conducted in countries facing high HIV prevalence. Several countries with the highest level of HIV prevalence ever recorded in Africa are among these: Zimbabwe, Zambia, Uganda, Malawi, and Swaziland. In sum, there are distinct variations in educational differentials, but they are seldom significantly different from the overall rate ratio, and when they are, they do not all go in the expected direction.

For both urban and rural respondents, we observe significant correlations between the random effects and the HIV prevalence lagged by 5 years (~ 0.3 and ~0.4) (UNAIDS 2012) as well as the overall level of mortality (~ 0.5). This confirms that the mortality advantage of education is reduced in high mortality and high HIV countries. It should be emphasized that there are large differences in the share of the population which remains uneducated in the surveys considered here. One could think that the mortality advantage of educated women is associated with the proportion of the population that is uneducated. However, we computed Spearman's rank correlation coefficients between the percentages and the two series of rate ratios and found no significant correlations.

The comparative advantage of women whose sisters had reached secondary education conforms to our expectations, but the opposite pattern observed in some DHS requires further investigation. It could point to a disproportionate underreporting of deaths by women with lower education, shifting the overall rate ratio upwards. It could also point to a detrimental effect of education on survival chances in high mortality countries –potentially related to the HIV epidemic. We investigate this further in two directions in the following sections. First, we analyze how the quality of reporting of deaths varies according to the educational attainment of respondents. Second, we evaluate how mortality gradients have changed over time in countries where several DHS have included a module on sibling survival.

4. Does the quality of death reporting vary across educational categories?

Because mortality gradients are analyzed from retrospective data, there is a concern that recall errors could be more pervasive among women with lower education. There is limited evidence on this issue, because previous assessments of the quality of sibling data were not disaggregated by the educational attainment of respondents. Using Pearson's chi-squared tests, we first observed that completeness of data on current age, age at death and the timing of death was associated with the educational level of respondents in most surveys. However, it is unlikely that mortality differentials are heavily distorted, because proportions of missing responses remain marginal. In 3 DHS out of 4, the age at survey was reported for more than 99% of surviving sisters, the age at death was reported for more than 96% of deceased sisters and information was available on the timing of deaths for more than 97% of cases.

We therefore use another standard indicator of data quality - the consistency between estimates from overlapping surveys - to look into this issue. In practice, we retain only periods for which at least two surveys overlap, and pool them together in one simple Poisson regression for each category of educational attainment. This corresponds to 98 surveys out of the 111 DHS analyzed in this paper (others surveys refer to countries with only one DHS with sibling histories or DHS conducted to far apart). In addition to the age, time period and country, the model also includes dummies for the time elapsed prior to the survey. The coefficients on these dummies quantify the decay in death reporting as we move further back in time. Further details on the methodology are provided in Obermeyer et al. (2010) and Masquelier *et al*. (2014).

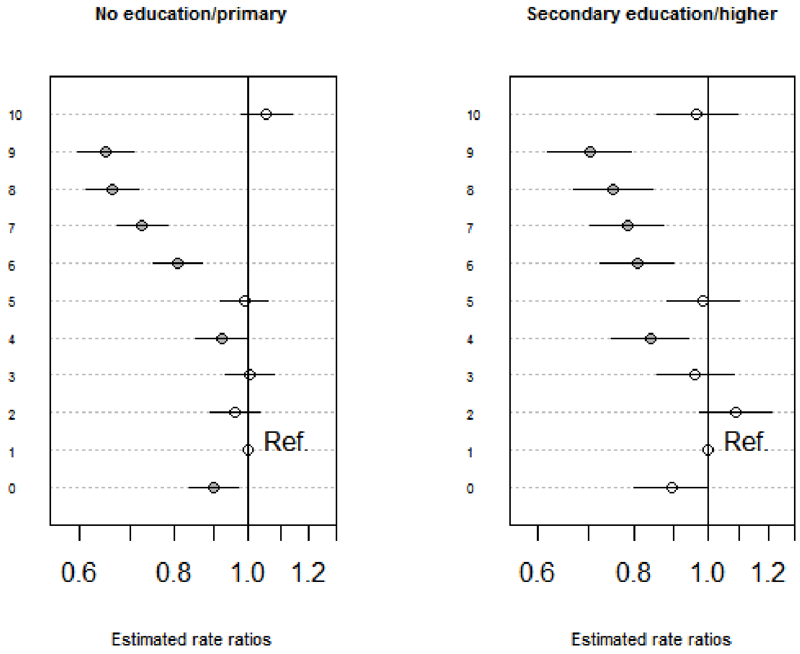

Figure 2: Mortality rate ratios in female mortality associated with the number of completed years prior to the survey. Reference category: 1 to 1.9 exact years before the survey. Estimates based on 98 DHS surveys.

Figure 2 displays rate ratios relative to the time prior to the survey, for 10 completed years before data collection. This plot can be read as follows: the rate ratio for 8 completed year prior to the survey are as low as 0.72 in the category 'no education/primary'. This means that about 30% of deaths were either unreported or displaced, when compared to 1 to 2 years before the survey. This is why we retained only the data for the period 0-6 years to estimate the mortality advantage of education, because the extent of death reporting does not vary much in this period. The pattern is also quite similar in both categories of educational attainment for these first few years. By contrast, the completeness of death reporting declines rapidly below 1 as the interval between deaths and the survey increases above 5, and the decline is slightly more rapid among women with less education. Of course, this approach only captures the underreporting *relative* to the period 1 to 1.9 exact years before the data collection. Reports of less educated women could be characterized by a larger underreporting of deaths unrelated to the time prior to the survey. This method also mixes omissions of siblings and displacements of deaths. This can be seen with the periods 5-6 and 10-11 years prior to the survey, which present higher rate ratios than surrounding periods, because of heaping on years since death. Yet, this approach provides limited support for the hypothesis that non-sampling errors are more pervasive in reports from women with low education when restricting the analysis to the data for the first few years before the survey.

5. Can DHS data support an analysis of trends in mortality gradients?

Finally, we use DHS surveys to investigate changes in mortality gradients for selected countries in which several sets of sibling histories have been collected. From the Poisson mixed-effects model described in section 3, we predict for each survey the life table probability of a woman dying between ages 25 and 40 by broad level of education (no education/primary *vs* secondary/higher). Again, to limit biases related to omissions of deceased siblings, we keep only the data for the first 6 years before data collection and assume that the mortality is constant over this period. Figure 3 presents trends over time for 9 countries: Cambodia, Indonesia, Cameroun, Ethiopia, Madagascar, Malawi, Namibia, Zambia and Zimbabwe. These countries were selected because they have conducted at least 3 DHS with a sibling module, and they correspond to varying levels of mortality (note the change in y-axis scale).

In the first 4 countries, we find the expected survival advantage of more educated women. This advantage is relatively small in Indonesia (with rate ratios around 0.8 for more educated women), it is more pronounced and stable over time in Cambodia and Bolivia (with rate ratios around 0.6 in both cases), and it has reduced as mortality rates have declined in Madagascar - except in the last survey.

We find this negative relationship between education and adult mortality in many others countries with relatively low mortality and no generalized HIV epidemic such as Peru, Philippines, Senegal, Nepal, and Dominican Republic.

The other 5 countries displayed in Figure 3 have faced a generalized HIV epidemic. Owing to the high fraction of mortality attributable to AIDS in these countries, we expected that mortality gradients would follow a common pattern, based on the literature on the link between HIV and education. Studies published on this topic prior to 1996 found either no association or a higher risk of HIV among the more educated, whereas more recent studies tend to find a lower risk of HIV infection among the most educated (Hargreaves et al. 2008). This is because highly educated individuals were more likely to engage in risky sexual behaviors given the availability of casual sex and the means to pay for it during the first stages of the epidemic (Gregson et al. 2001). Forston (2008) also found a robust positive education gradient in HIV infection, after controlling for a rich set of confounders and non-response bias in HIV testing, where adults with 6 years of schooling were more likely to be HIV positive than adults with no education. Later on, it seems that more educated people have been responding more adequately to behavioral change programmes (Glynn et al. 2004). As a result, we should observe a reversed gradient between education and mortality in the beginning of the epidemic, followed by a return to small mortality differentials that tend to favor the more educated. We could argue that this pattern is observed in Cameroon, Malawi and Zimbabwe.

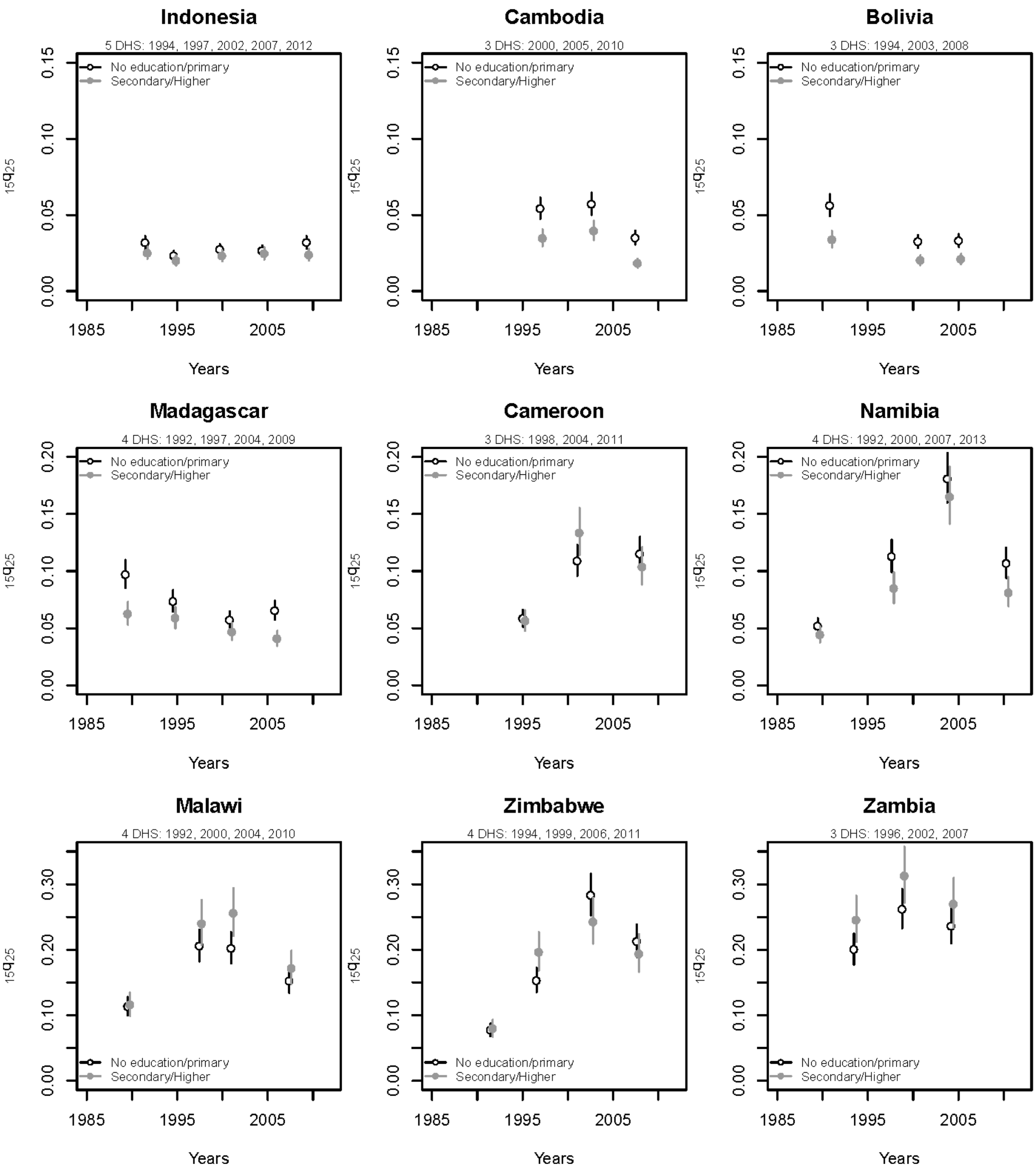

Figure 3: Trends in the life table probability of a woman dying between ages 25 and 40 by level of education.

However, when looking at the set of trends presented in Figure 3, there is a great heterogeneity in the magnitude of inequalities in mortality and in their direction, and no systematic pattern emerges. In

Namibia, there has been a massive rise in mortality among young adults, and more educated women have benefitted from a survival advantage throughout the period. In Zambia, a reversed gradient is observed, with more educated women facing relative excess risks of dying (estimated at about 1.2 in the three surveys).

6. Discussion

The lack of research on size of mortality differences associated with educational level is striking in low- and middle-income countries. This is in part due to the paucity of data on adult survival, but perhaps some data sources regularly used to estimate mortality trends should be more systematically explored for the study of health inequalities. We argue here that sibling histories can help to document empirically the relationship between educational attainment and the level of adult mortality. Because of the absence of information on the socio-economic characteristics of siblings, the critical assumption is that educational outcomes are correlated within sibships, therefore the level of educational attainment of the respondent acts as a proxy for the level of educational attainment of her siblings. In most DHS, we have confirmed that the correlations of schooling outcomes among sisters are so high that this assumption seems to hold, at least for females. Further research is needed to evaluate if the method performs well for estimating differentials in male mortality. Including in DHS questionnaires a question to link sisters living in the same household, and additional questions on basic socio-economic characteristics of the siblings would also be beneficial to have direct measurements and reduce misclassification errors. These errors will reduce the size of the differentials based on proxy reports, and sensitivity analyzes should be undertaken to quantify their impact.

When looking at our second set of results, i.e. the ratios between mortality rates of women whose sisters had at least reached secondary school to rates experienced by women whose sisters were less educated, the protective effect of education is observed in a majority of surveys. However, a puzzling pattern is seen in a dozen of surveys, where more educated women appear to be disadvantaged, especially when analyzing reports from rural respondents. Surveys conducted in Cameroon, Zimbabwe, Zambia, Malawi and Uganda were characterized by the highest excess risks of dying among the more educated women. These countries have all been affected by large HIV/AIDS epidemics, albeit to a varying degree.

These reversed gradients could be attributed to several factors, such as a disproportionate underreporting of deaths by women with lower education and/or a detrimental effect of education on survival chances in high mortality countries – with this effect largely due to the impact of the HIV/AIDS pandemic. To explore these two issues further, we measured the completeness of data on ages and the timing of death and found an association with the respondent's level of education in many DHS, although the data remain remarkably complete in most surveys. When evaluating the consistency of mortality derived from successive surveys, we found evidence of a massive underreporting of deaths. When limiting our analysis to the 0-6 years period before the survey, there was no sign that underreporting is more pervasive among women with lower education. In sum, differentials in non-sampling errors are unlikely to be large enough to invalidate the approach.

Finally, this study presented trends in female mortality by level of educational attainment for 9 countries with several DHS (Indonesia, Cambodia, Bolivia, Madagascar, Namibia, Cameroon, Malawi, Zambia and Zimbabwe). The results point to a large heterogeneity which is both a function of the overall level of adult mortality and the stage and size of the HIV epidemic. In countries with relatively low level of adult mortality such as Bolivia and Madagascar, we found the expected negative gradient. Some countries presented instead an evolving gradient which points towards the dynamic and heterogeneous role of HIV as a confounding factor vis-à-vis the positive relationships between educational attainment and survival gains.

Our results need to be interpreted with caution due to the existence of four important caveats. First, confidence intervals around adult mortality estimates often overlap, despite wide gradients in mortality levels. Hence, DHS data lack statistical power and provide at best an approximate estimate of the extent and direction of the observed gradients. However, sibling histories can provide a rough idea of the extent and direction of the gradients. This, in itself, is a major contribution, considering the data constraints in this area. Second, selection biases could be introduced by the retrospective collection of data, and they could affect the mortality rate ratios. We discussed these biases in greater detail elsewhere (Masquelier 2013). We assumed here that mortality is unrelated to the number of sisters surviving in adulthood. However, some clustering of adult deaths within sibships could distort the estimated gradients in mortality, even in the absence of an association between mortality and sibship size. Third, we cannot really unpack the reasons and mechanisms behind such gradients because no data on causes of deaths or behaviors of siblings is collected as part of existing DHS surveys. Another survey program, the World Health Surveys, has integrated in the module on the survival of siblings some questions about the circumstances of siblings' death and some symptoms, and future research could make use of such data. Fourth, the data does not allow us to establish whether these gradients are truly associated with education or other characteristics which are closely related to education such as urban-rural residence or income.

Cognizant of these limitations, the use of the sibling method represents a valid contribution over the techniques used so far to estimate the size of mortality differences associated with education in developing countries. In particular, sibling histories could inform the reconstruction of population trends and projections by level of educational attainment (Lutz and Goujon 2001). These analyses should at least take into account temporal changes in the relationship between education and mortality, which is conditional on context as well as patterns of causes of death. Further research should focus on the validation of the analysis presented in this paper with supplementary datasets for specific countries that would allow triangulation of results. More generally, an understanding of the causal mechanisms that drive the relationship between education and mortality and lie behind such gradients is still lacking for developing countries.

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